

NEW YORK UNIVERSITY-BELLEVUE MEDICAL CENTER
NEW YORK UNIVERSITY COLLEGE OF MEDICINE
550 FIRST AVENUE, NEW YORK 16, N.Y.

INSTITUTE OF
RECONSTRUCTIVE PLASTIC SURGERY

December 15, 1958

Dr. Joshua Lederberg
Professor of Medical Genetics
University of Wisconsin
Madison
Wisconsin

Dear Dr. Lederberg:

I have just read in a news report of your new hypothesis on the mechanism of antibody production given at a Harvard University Medical School Lecture recently. It is strongly possible that you may not remember me personally, but I was a member of the 1948 medical school class at Columbia University. I remember you well when you started off as a freshman medical student with us, only to leave and make a far better choice in your career than some of our classmates.

Before I get into the substance of this letter, I would like to heartily congratulate you, not only for your receiving the Nobel Prize this year, but for your many previous contributions to the field of medicine and genetics which I have been following closely during their development over these past 10 years or more.

I am writing this letter specifically as the associate editor of the Transplantation Bulletin, to request a favor of you if you are able to fulfill it. As you know, the work of Sir MacFarlane Burnet has interested many of us involved in the field of tissue transplantation and particularly in our studies of immunologic acquired tolerance. The field of transplantation is particularly in need of a better association with geneticists and immunologists who can lend to us more accurate explanations of some of the immunologic phenomena involved in our work. Any new theory on the mechanism of antibody production, therefore, is certainly of great interest to us. The brief abstract of your lecture which was given in Scope Weekly, I found extremely interesting, particularly in view of its references to immuno-genetic differences.

ROGERS, B.O.

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In our own investigations, in one respect, many of us have been too rigid in our attitude about genetic control of transplantation experiments, and in another respect, some workers only too often are willing to dismiss the whole concept of genetic determination entirely. Would it be asking too much, therefore, since your lecture apparently dealt with immune tolerance, for you to write for us a brief review or a lengthy one, if you should so wish, that we can publish in the next issue of the Transplantation Bulletin as a privileged communication to the Bulletin? We would prefer that you be as free-wheeling as you would like to be in any presentation of this material, knowing that most of us who read the Transplantation Bulletin and those of us involved in the transplantation of tissue, especially in the study of immune tolerance, would certainly understand that your hypothesis would still require verification, but at this time might stimulate more of us to think with a fresh outlook on the many phases of this problem.

If your Mueller Memorial Lecture at Harvard is in a manuscript form and could possibly be broken down into a shorter review, I am sure that I could influence the other editors of our Bulletin to publish the shorter review in toto. It would, thus, reach the greatest number of people interested in the field of immune tolerance today.

Hoping that you will be able to reply affirmatively to this request, and wishing you and your wife every success that it is possible to achieve in your careers, I am,

Sincerely yours,

Blair Rogers

Blair O. Rogers, M.D.

BOR/ma

C. 5. Enclosed are a few of our recent papers, including a bibliography with references to acquired tolerance.